XIII. *Electro-Physiological Researches.—Fifth Series.* Part I. *Upon Induced Contractions. By Signor* Carlo Matteucci, *Professor in the University of Pisa, &c. &c. Communicated by* Michael Faraday, *Esq., &c. &c.*

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In my third memoir upon Induced Contractions, published in the Philosophical Transactions for 1845, at p. 303, after having discussed at length the various hypotheses which appear to offer an explanation of this phenomenon, I was led to conclude that it was due to nervous influence acting through the muscle during contraction; that, in a word, it was to be referred to a kind of nervous induction. In effect, I detailed a number of experiments in that memoir, which prove that there is never any manifestation of the signs of an electric current during the contraction of the muscles; thus in exciting contractions in one of my piles composed of muscular elements, in which the circuit was completed by the galvanometer, the signs of the muscular current were never perceived to increase. Finally, I have shown that the induced contraction is propagated through a coating of turpentine, which is of a nature sufficiently insulating to arrest the passage of any electric current.

I was therefore warranted in deducing from these phenomena that in the muscles which contracted, and so produced the induced contractions, there was never any electric current generated, and that therefore the induced contraction could not be explained by a reference to any such agency.

I will now cite some new researches instituted with a view to the discovery of the nature of the phenomenon of induced contractions, which is so obscure and at the same time so important.

Observing that the slightest discharges from the jar, inappreciable by the most delicate of our electroscopes, are invariably sufficient to excite violent contractions in the frog, it appeared agreeable to analogy to suppose that the cause of induced contractions might reside in a discharge similar to that of the jar, taking place in the muscle in the act of contracting. If that had been the case, it would no longer have been a matter of surprise that the galvanometer should give no indication during muscular contraction.

I began my researches by ascertaining whether, by passing very slight discharges of the jar through the muscular masses, contraction was excited in the galvanoscopic frog, the nerve of which touched the muscle traversed by the discharge. In effect, the galvanoscopic frog never fails to contract under the influence of extremely feeble discharge, such as are elicited after a very small jar has been discharged two or three

times with a metallic arc. It is needless to say that the galvanoscopic frog remains insulated in this experiment, merely having its nerve resting upon the muscle.

In like manner it must be observed, that in the above experiment muscles were employed which had ceased to contract during the discharge, so as not to have any induced contraction.

It must then be admitted that, in spite of the good conductibility of a muscular mass, a part of the discharge always escapes to the surface of the muscle and so traverses the nerve of the galvanoscopic frog.

The occurrence of this phenomenon is still more remarkable when the nerve of the galvanoscopic frog is placed upon a metallic surface through which the shock is passed.

The phenomenon may very well be produced by passing the discharge through a plate of tin or gold upon which the galvanoscopic nerve is laid out; only in this case we have not so many successive shocks producing contraction as with the muscular arc; evidently because the jar is much more perfectly discharged through a metallic conductor than through the muscle.

The next step therefore to be verified was whether a very slight discharge passed through a muscular mass would still excite the galvanoscopic frog to contraction, even when an insulating medium was interposed between the surface of the muscle and the galvanoscopic nerve. It also suggests itself naturally to the experimenter to employ in every experiment the same insulating coatings which are known to destroy induced contractions.

In this view I have covered the muscular mass with a coating of turpentine, and laid the galvanoscopic nerve upon it. On passing very slight discharges, the galvanoscopic frog invariably contracts. I have repeated this experiment very frequently, and the only difference that I have remarked has been in the number of successive shocks acting through the turpentine. The shocks are generally fewer than when there is no turpentine. Thus with a very small jar, and without the turpentine, five or six, and even as many as ten successive shocks may be obtained, and which cause the contraction; and when the turpentine has been interposed, the shocks are from four to six. The depth of the insulating stratum was always such that a current from a pile of fifteen couples of plates could not penetrate it.

I have gilded the muscular conductor, and stretched the galvanoscopic nerve upon its surface; on passing shocks from the small jar I still had contractions in the galvanoscopic frog. In this case also it is to be observed that contraction never went beyond the second or third discharge.

Finally, on interposing very fine plates of mica between the surface of the muscle and the galvanoscopic nerve, the frog contracted at two or three successive discharges from the small jar. I will here call attention to a fact somewhat remarkable, which has always occurred in these experiments: on passing the first shock of the jar accompanied by the spark, the contraction of the galvanoscopic frog almost invariably fails.

What inferences may be drawn from these experiments to aid our decision as to whether the induced contraction admit of an explanation on the supposition of an electric discharge similar to that of the jar, and taking place during the contraction of the muscle?

Before examining this point, I will relate a few other experiments upon the induced contraction which throw some light upon the interpretation of this phenomenon.

In a living dog, the spinal marrow (deprived of its membranes), the brain, the sciatic nerve, and the muscles of the thigh were all laid bare. Galvanoscopic frogs were placed in the usual manner upon these several parts, and at the same time the animal was irritated either by squeezing his paw, or by wounding the coverings of the spinal cord. The muscles of the thigh were thrown into violent contraction, and the animal howled from pain. In the meantime the galvanoscopic frogs placed upon the muscles in contraction alone exhibited the phenomenon of induced contraction. The same experiment was instituted upon a rabbit, and was followed with the same results. In like manner galvanoscopic frogs were arranged upon the abdominal viscera of a living rabbit while an electric current was passed along the pneumogastric nerves, or through the solar plexus. Never in the above experiments did the galvanoscopic frogs exhibit any indication of induced contractions. Now if it be borne in mind that in the experiments just related, while the animal shrieked with pain and was convulsed, there certainly was a current of that force, whatever may be its nature, which we term nervous, and yet no contraction in the galvanoscopic frogs placed on the nervous structures, we are forced to conclude *that the phenomenon of induced contraction belongs exclusively to the muscle in the state of contraction.*

With regard to the question whether the induced contractions admit of being explained by the supposition of an electric discharge elicited in the act of the contraction of the muscle, we are obliged to confess that the experiments recorded in the commencement of this memoir (and which are the only ones that in the present state of the science could have been instituted with a view to the decision of the question) are insufficient to afford a satisfactory solution.

Admitting that an electric shock analogous to a very slight discharge of the jar did actually take place during the contraction of the muscle, we should have no other means of ascertaining its existence than that of the induced contractions, the origin of which we seek to discover.

In effect, not one of our electroscopic instruments is capable of disclosing to us the existence of an electric discharge, such as that given by a very small jar previously discharged three or four times successively with a metallic conductor.

The galvanoscopic frog alone can indicate the existence of these discharges.

Respecting the influence of the coating of turpentine upon the two phenomena under consideration, induced contractions, and discharge through a muscle which acts likewise upon the galvanoscopic frog, we have seen that no difference existed between the two cases.

A difference, however, does exist on interposing a lamella of gold or of mica between the galvanoscopic nerve and the muscle: these lamellæ prevent the induced contractions without destroying the effect of the discharge of the jar. I am far from concluding from the above fact that the two phenomena may not have the same origin. We have no precise knowledge as to why the discharge of the jar can be very slight without its ceasing to excite contraction in the galvanoscopic frog; and we have seen in those cases in which the lamellæ of mica or of gold were interposed, that the number of successive discharges from the jar, which acted upon the galvanoscopic frog, was always less than when no lamellæ were interposed.

We must therefore pause a little before we can establish the fact of induced contractions not being due to an electric discharge produced during the contraction of the muscle; and since it is impossible for us to solve the question by direct experiment, let us be guided by such analogies as appear to have the best possible foundation. I purpose examining this field of investigation in a general summary of all my electro-physiological researches, which summary I hope soon to be able to complete.

PART II.

Upon the Phenomena elicited by the passage of the Current through the Nerves of a living Animal, or an Animal recently killed, according to the direction of the Current.

In my fourth memoir I took great pains to prove at length by the aid of experiment, that the electric current transmitted along a nerve modifies the excitability of the nerve in a manner differing widely according to the direction of the current; thus the direct current rapidly exhausts this excitability, while the inverse current increases it. Starting from this fact, I hope that I have given a satisfactory theory of electro-physiological phenomena.

Among the different experiments described on this head, I indicated one in particular which appears very singular, and which I have since studied in all its bearings. The frog prepared in the ordinary manner, and divided in the pelvis, is placed astride between two little glasses in which the reophores of a FARADAY's pile of fifteen or twenty elements were immersed. It is evident that one of the limbs is traversed by a direct current and the other by an inverse current. It is unnecessary to describe here minutely the phases of the phenomena which present themselves during twenty or twenty-five minutes.

In the first place, the two limbs contract both on closing and on opening the circuit, after which there is contraction of the limb traversed by the direct current on closing the circuit; and the other limb contracts on breaking the circuit: finally, only one limb contracts, viz. that of the inverse limb on the cessation of the passage of the current. On keeping the circuit closed for some minutes, we invariably remark that the inverse limb, which contracts on breaking the circle, is seized with a permanent contraction of a decidedly tetanic character. This phenomenon is of importance,

as it indicates an intimate connection between nervous influence and the action of the electric current according to the direction of the latter.

I have therefore exercised the greatest care in studying this phenomenon; and if I have not been able to show the connection in all its evidence, and to express it with that degree of simplicity which is the characteristic of physical laws, I venture to hope that this failure will in part be attributed to the obscurity in which the subject is involved, and that some degree of favour may be accorded to the efforts which I have made in this direction. It is a very rare circumstance to find a frog that does not present the phenomenon which has been already described; and in particular those frogs which pass several days in winter without nourishment, never fail to manifest it. In this case they appear more disposed to become tetanic; and, in effect, they do almost all become so when they are prepared by dividing their spinal marrow, and remain so during some seconds. The phenomenon in general manifests itself after the current has been passed for twenty-five or thirty minutes. The tetanic contraction lasts a very long time, and it often happens that when this has ceased, there are twitchings in the limb from time to time. These phenomena equally occur on passing the current through the nerves without its traversing the muscles. In like manner they may be elicited in the living frog, only in this case the tetanic contraction lasts a much shorter time. The phenomenon never occurs when the current acts only on the muscle, which it may be made to do by disposing the frog in the ordinary manner, without having divided the pelvis. On the other hand, it presents itself after the spinal marrow has been destroyed.

It is not essential to the manifestation of the phenomenon in question that the muscles should be thrown into contraction at the commencement of the passage of the current, which may easily be seen by closing the circuit by the aid of a bent metallic conductor ending in some paper which *slowly* imbibes a certain quantity of water. If, instead of arresting the current by removing one of the reophores of the pile, a metallic arc be introduced between the two glasses, the tetanic contraction occurs just the same. But if to the end of this curved rod be attached some paper, so as to occasion the passage of the current to cease more slowly as regards the frog, then instead of one tetanic convulsion we have a series of contractions succeeding each other at short intervals of time.

In whatever manner the current through the nerve of the inverse limb is arrested, the tetanic contraction is excited. It suffices for this to moisten the nerve with a large drop of water, or to double it back upon itself for the contraction to take place; while this does not occur if the current is arrested for the muscle. This is easily effected by bringing the reophore of the pile, while the circuit is still kept closed, in contact with the thigh at the point where the nerves immerge.

The following experiment proves still more clearly the part which the nerve plays in this phenomenon. If, while the circle remains closed, and it has been previously ascertained that the tetanic contraction will follow the opening of the circuit, the

nerve be divided rapidly at the precise point where it enters the muscle, the limb is thrown into contraction without its remaining in a state of tetanus. But if, instead of this, the nerve be divided higher up near to its issue from the spinal marrow, then the tetanic contraction takes place as usual.

We will next consider the circumstances which modify or destroy this phenomenon. None of those frogs which were killed after the administration of large doses of morphine, so as to manifest every symptom of narcotism, ever exhibited tetanic contractions. The same thing occurs with frogs which have been made to support heavy weights with their legs. The circumstance which suddenly puts a stop to the tetanic contractions is the passage of the inverse current, the same, that is to say, by which the phenomenon is produced. Generally two or three seconds after the circle is closed again, the limb falls into its natural posture. If, on the other hand, the current merely ceases to pass, it will in general be three minutes before that happens; and the same is the case pretty nearly if the direct current is passed. To obtain these phenomena with great precision, care must be taken not to reverse the direction of the current upon the same nerve several times successively.

I was desirous of trying whether the species of tetanic contraction, or rather of corpse-like rigidity with which a muscle is seized after a shock from the jar, was dissipated by the passage of the current, direct or inverse, through its nerve. This is not at all the case, and whenever it has occurred, the passage of the current has not produced contraction.

I will here state that, acting with the electric current upon the nerves of living warm-blooded animals, as rabbits and dogs, a phenomenon analogous to that which we have studied in the frog is very clearly seen to follow the action of the inverse current; only it is observable that in these animals the tetanic contractions last a much shorter time, especially if the vitality of the animal is very great; the same fact besides is verified in the frog.

How is this tetanic action produced? It is easy to convince oneself, if any doubt could be entertained upon the subject, that there is no electricity rendered latent either in the nerves or in the muscles by the passage of the inverse current. My endeavours to discover signs of any, by the aid of the condenser, have been entirely fruitless. Likewise there are no signs on opening the circuit of any electric current in circulation. I have made myself quite certain of this fact by means of the galvanometer, employing at the same time a pile of tetanized frogs.

The phenomenon which we have studied thus at length belongs clearly to the nerve, and takes its origin from the relation, the nature of which is yet undetermined, which exists between nervous influence and the action of the electric current according to the direction of the latter.

It has been distinctly proved in the fourth series of these electro-physiological researches, that the passage of the direct current destroys the excitability of the nerve, and that this is not the case with the inverse current, which acts in a contrary

manner. The direct current acts by exciting, when it commences, a contraction which we know to be stronger than that produced in the same case by the inverse current. A limb traversed by the direct current may be compared to a limb fatigued by repeated efforts. The inverse current may be supposed to act in an opposite manner, and during its passage the nervous force might accumulate in the nerve.

I prefer, however, suppressing considerations of this nature on account of the vague and uncertain impression which they are calculated to leave in a reflecting mind.

It may not, however, be without some utility to close my electro-physiological researches with an attempt at embracing, under some general views, the phenomena of muscular contraction, of the production of electricity in fish, and of the relation between the electric current and nervous influence.

I would here recall the facts discovered by M. Longet and myself, on treating the motor nerves with the electric current. Monsieur Longet and myself discovered that the action of the electric current upon these nerves was precisely the opposite of its action upon the mixed nerves.

The direct current acting upon the motor nerves, determines the contraction in the second period of the excitability of these nerves when it ceases to pass, and the inverse current produces it when it begins to pass. It is natural to think that the phenomenon of tetanic contraction would be produced by the cessation of the direct current when the motor nerves are acted upon. In connexion with this subject, I will here relate a phenomenon which I have lately discovered, and which appears to me to be worthy of remark. With the aid of a wheel armed with insulating metallic teeth, similar to that of Mons. Masson, I passed a current in a frog prepared in the manner described above, that is to say, like that placed astride between two glasses. In this manner I pass the current 500 or 600 times in the frog. After this, on attempting to interrupt, and then again establish the circuit, the following phenomenon strikes the experimenter. The inverse limb contracts on closing the circle, and the direct limb on opening the circle. On leaving the circle closed for some minutes, the ordinary phenomena—the opposite of the preceding—appear; that is to say, the direct limb contracts on closing, and the inverse limb on breaking the circle. It is very difficult to reproduce these phenomena in the same frogs, on account of the extreme feebleness produced by the numerous contractions.

The above fact establishes a fresh connection between nervous influence and the passage of the electric current according to the direction of the latter. I will return to this subject in the general views which I announced above.

Pisa, December 1846.